which, if judged from our point of view, would produce a strange impression; but these he puts in their right place as elements which mark definite stages of evolution. Too few references are given in the notes, but the information may be accepted as generally trustworthy, having been for the most part derived from the statements of travellers and missionaries during the first half of the present century.

Tongues in Trees and Sermons in Stones. By the Rev. W. Tuckwell. (London: George Allen, 1891.)

THE greater part of this volume is occupied with papers which bear the general heading "Tongues in Trees." They are interesting essays, written in a clear and pleasant style, and presenting in a fresh light a good many "facts in flower-lore" Among the subjects are tree worship, tree myths and superstitions, plant-names of persons, places, and seasons, plant monsters, gardens, and plant literature. The fact that there may be "sermons in stones" is illustrated by a short paper on sundials, reprinted from the *Gardener's Chronicle.*

Supplement to Euclid Revised. By R. C. J. Nixon, M.A. (Oxford: Clarendon Press, 1891.)

IN this little book the author has placed before the student a few theorems on the more modern geometry of the triangle. The points touched upon relate to the Lemoine and Brocard points, lines, and circles; and a study of these will serve as a good introduction to many of the more advanced theorems. The proofs are concise and complete, and a few exercises are added which are more or less dependent on them.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Alpine Flora.

FOR some years past we have endeavoured to make as large a collection as possible of the more interesting Alpine plants at Kew. And I am glad to take the opportunity of acknowledging the invaluable assistance which we have received from Mr. G. C. Churchill, who has spared no pains on our behalf in corresponding with collectors in almost every mountainous district of Europe.

The observation of the Kew collection has led me to a few conclusions which have some bearing on the questions raised by Mr. Cockerell and the Rev. George Henslow.

What are called Alpine plants appear to me to fall into two distinct classes: (1) those which have a permanent adaptive habit, and (2) those which have a habit due merely to the local conditions of the growth of the individual plant and not maintained by it if those conditions are altered.

The peculiarities of the former class I am disposed myself to attribute to natural selection. Anyone who supposed that Alpine plants were particularly "hardy," would find himself very rapidly disabused by attempting to cultivate them. I have no definite data on the subject; but I am inclined to think that for the most part they are intolerant of very low temperatures. They certainly are extremely impatient of humidity during the period—a comparatively long one—when they are not in active growth. We are for these reasons, paradoxical as it may seem, obliged at Kew to winter our large collection in frames under glass. The reason is that in Nature, except for a short time, Alpine plants are covered with snow, which keeps them dry and protects them from a very low temperature. Last summer, accompanied by the foreman of our herbaceous department, Mr. Dewar, I crossed the Zwischbergen Pass from the village of Simplon to Saas. This was late in July, and on the south side we crossed a wonderful area of Alpine vegetation from which the snow had only just melted; I suppose that by the end of September it would be covered up again. I presume the ground would remain permanently frozen at no great depth. The plants were therefore reduced to growing in the thin film of soil warmed during exposure by the sun's heat, and this would be, for the greater part of the year, sandwiched between deep overlying snow and the frozen subsoil. No plants which had other than a close dwarf habit would seem to me to have a chance of existing under such circumstances.

On the lower slopes of the valleys of the Alps there is a profusion of interesting plants, which, though not so dwarf as those of higher levels, still derive a good deal of their charm from the more or less compactness of their growth. It is a matter of general experience that, when many of these are transferred to a country such as England, they develop into a coarse weedy habit which deprives them of a great deal of their interest. I am not satisfied that a richer soil in cultivation affords the explanation of this. I am disposed to think that the conditions of solar illumination in the Alps are a much more important factor.

Every practical gardener knows that if you want to maintain a desirable stubby habit in a decorative plant, and to keep the vegetative structures in proper subordination to the flowers, you must expose your plants as freely as possible to sunlight. This is not, however, a question of promoting assimilation, but of preventing elongation and extension of parts already formed. There is probably much the same amount of material in the stubby and the lanky plant.

Now plants which grow at high levels in the Alps are *above* a great screen of aqueous vapour. They therefore get a larger share of the more refrangible rays of sunlight than those that grow nearer sea-level. And it is precisely these rays which it is known inhibit growth or rather extension in length.

But though, as I believe, this restraining and, if you like, dwarfing action has been going on for ages, it is not my experience that the effect is permanently impressed on the species. I wish it were.

I have met with one plant in which I thought that I had got a case of the transmission of an acquired habit. Mr. Churchill procured for us seeds of *Arabis anachoretica*, "a form of *Arabis alpina*, L., with thin tissue-papery leaves, growing in hollows of the rock where neither sun nor rain reach it, just as *Saxifraga arachnoidea*, as also *Heliospermum glutinosum*, and *Zahlbrucknera paradoxa*, all which have very thin tissue-paper leaves." Alas, on cultivation at Kew it reverted forthwith to commonplace *Arabis alpina.*

Mr. Henslow remarks that "the action of the environment on plants is a thing which can be tested," while the accumulation of many useful variations cannot. Hence he appears to infer that Darwinism is an *a priori* theory, while neo-Lamarckism is not. If he rests this conclusion on facts drawn from the study of the Alpine flora, all I can say is, that while horticulturists are obtaining dwarf varieties every day by selection, I am not aware of any one which can be demonstrably shown to be the product of the action of external conditions.

W. T. THISELTON-DYER.

Royal Gardens, Kew.

Neo-Lamarckism and Darwinism.

MR. COCKERELL'S reply to my remarks is interesting, as I cannot find anything in it with which I do not cordially agree. A few words, however, may clear up matters.

(1) I referred only to dwarfs on high Alpine and Arctic regions. *These* are due to cold (see Grisebach, "La Vég. du Globe," i. 49-51; Verlot, "Sur la Production et la Fixation des Variétés," p. 36).

(2) I am quite convinced of the hereditary character of "nanism," as also of "gigantism," as well as of other results of the direct action of the environment on plants; and am glad to find Mr. Cockerell has come to this conclusion from direct observation. As an example of hereditary hypertrophy, the parsnip called "The Student" was raised by Prof. Buckman from seeds of wild plants in 1847, and it is still sold by Messrs. Sutton as their "best variety." On the other hand, Mr. Cockerell is perfectly correct in admitting the, so to say, plasticity of dwarfs. Indeed, my own experiments with aquatic plants lead me to wonder why some species are, as a rule, so fixed as they generally appear to be.

(3) Tall plants are certainly liable to be injured by winds; as I added, "as occurs at lower altitudes"; but are there any such among the dwarfs *above the tree line?* If not, natural

© 1891 Nature Publishing Group

selection has no tall varieties to work upon; or, if natural selection has eliminated them at that altitude, why has it not done so at a lower one? M. Bonnier's experiments go to show that they will not form there (see his figures and descriptions in "Rev. Gén. de Bot.," ii. p. 528). All I ask for is, that Mr. Cockerell's supposition should have some *facts* to rest upon.

(4) I did not question the fact that dwarf Alpine plants may have additional warmth from their close proximity to the soil. De Candolle observes: "Il en résulte que la couche superficielle du sol doit en général être plus chaude sur les montagnes que dans la plaine, la moyenne extérieure étant supposée semblable" ("Géog. Bot.," ii. p. 260). Indeed, I have grounds for suspecting that the prostrate condition, common in Alpine and other exposed plants, is generally, a result of the direct response of thermotropism. In support of this statement, I will give two out of several observations. *Malva sylvestris*, when growing freely, surrounded by a dry hard soil which is readily heated, develops a prostrate form; but it grows erect when in a damp soil and surrounded by other plants. Bluebells often lay their first leaves flat on the ground. The temperature *on* the soil and under such a leaf was 47°·5 at 10 o'clock this morning (April 15). The plant is growing in shade and in damp soil. The temperature of the air 3 inches above it was 44°·5. Lastly, the temperature *under* the flat leaves of a plantain, growing in short grass covered with dew, was 52°·5, while at 3 inches above it, it was 49°·5. What I questioned was the right of building an argument upon a statement, when the opposite condition of great cold arising from radiation at night is ignored. Thus, De Candolle observes:—"Plus on s'élève, moins les rayons solaires sont interceptés par l'atmosphère, plus aussi la déperdition de chaleur par le rayonnement nocturne et par l'évaporation est considérable" (*l.c.* ii. p. 259).

(5) Individual plants may be sheltered; but is not exposure the *prevailing* feature in high regions? Such is, at least, what I have observed.

(6) "Those plants which naturally grow quickly . . . . would survive." Quite true; all I maintain is that the primary cause is not *internal*, as "naturally" seems to imply, but *external* to the plant. That is, the total amount of heat and light received by the plant in a comparatively short time is what induces the rapid development of high Alpine and Arctic plants. But, on the other hand, the responsiveness of plants is not absolutely alike in all cases; therefore we have enough evidence to draw the deduction which I will express in Mr. Cockerell's words: "Given such variations [in the responsiveness to climate], is it not obvious that natural selection would preserve the more rapidly-maturing kinds where the summer was short?" Certainly, as M. Verlot shows (*l.c.* p. 48), what horticulturists are familiar with, that precocious and late varieties are hereditary.

Mr. Cockerell's reply does not combat my sole contention, that it is unscientific to form theories of what "may be" and "might be," when *no, or insufficient, facts* are brought forward on which the supposition is based. There may not be a sufficiency to justify one in unhesitatingly accepting any particular theory; but all I ask for is something *positive*, or well-determined *facts*, upon which a hypothesis may be based. Science is not advanced, but hindered, by "may be's" and "might be's."

GEORGE HENSLOW.

Co-adaptation and Free Intercrossing.

As it appears to me, from his reply, that Prof. Meldola's views on the subject of "co-adaptation" are really the same as my own, I write once more in order to point out the identity.

In the first place, I of course agree that "every [considerable] variation, of whatever kind, must of necessity entail numerous structural and functional modifications"; and therefore "that there is not any 'fortuity' in the supposition that certain variations, A, B, C, D, &c., may arise in the same individual." "But," as Prof. Meldola continues, "the case we have to consider is that in which the variations, A, B, C, D, &c., are not [thus] physically or physiologically correlated; but in which they are supposed to be 'independent variables.'" Now, this being the only case we have to consider, Prof. Meldola says, "The chances against such variations occurring in any one individual are, I concede most fully, 'infinity to one.'" And lower down he as fully concedes the consequence, that if it be supposed necessary for the utility of A that A should occur in association with B, C, D, &c., then the chances must be infinity to one against the perpetuated existence of A. But this is all that Broca's and Spencer's "difficulty" from co-adaptation alleges; and, as I said in my last letter, if it is to be met, it can only be so by denying that any cases do actually occur in nature where the utility of A is thus dependent upon the association of A with the other independent variables, B, C, D, &c. Accordingly, this is the way in which Prof. Meldola's reply does meet the "difficulty." For he concludes—and very properly—"if it be said that A cannot exist by itself, but that A + B is the only form capable of existence, then it remains for those who make this assertion to prove that A and B were coincident in time and space *ab initio*"—*i.e.*, to show cases where the utility both of A and of B is dependent on the association A + B. And he goes on to add, "that the particular case of co-adaptation quoted by Mr. Pascoe [*i.e.* that of the giraffe] may resolve itself into a 'confluence of adaptations.'" But, if so, *cadet quæstio* as regards this particular case. And similarly, he continues, as regards all other possible cases; for the next sentence is,—"In fact, I do not think it would be going too far to put forward the proposition that all cases of co-adaptation may be ultimately explained in the same way, *i.e.* that they arise from the coalescence (by intercrossing) of *n* modifications each *useful* (not useless) in itself, and acquired at successive periods in the phylogeny of the race."

Thus Prof. Meldola meets the "difficulty" in the way which I believe is the only way that it can be met, viz. by denying that, as a matter of fact, any cases of "co-adaptation" (as distinguished from a "confluence of adaptations") ever do occur in nature. This, however, raises a distinct question. The issue between him and those who advance the "difficulty" will now no longer be logical, but biological. And, as stated in my previous letter, I do not intend to go into this distinct question. My only object has been to show that, unless the supposed fact of co-adaptation be denied, any appeal to the analogy of artificial selection is illogical; while, if this supposed fact is denied, any appeal to the analogy—then "real" enough—is superfluous. For, as I said before, it is self-evident that if each of the independent variables is of utility *per se*, natural selection will, sooner or later, blend them all together through free intercrossing.

Thus, with regard to the matter of co-adaptation, it appears that we are in complete agreement. Unless any given case—such as that of the giraffe—is denied to be a real case of co-adaptation, Prof. Meldola "fully concedes" that the chances against its evolution by natural selection alone must be "infinity to one," and, therefore, that the analogy of artificial selection as regards that case would be false. I am sorry to observe, however, that such agreement does not appear to prevail with regard to the distinct and much more important "difficulty of the swamping effects of intercrossing." For in his last paragraph Prof. Meldola ranges himself on the side of the "neo-Darwinians" as regards this difficulty, remarking, on the one hand, that he does not believe it to be "real," and, on the other hand, that it has been "amply treated of by Mr. Wallace and others." The difficulty here alluded to is to conceive how it is so much as logically possible for natural selection *alone* to preserve and accumulate incipient variations in the face of free intercrossing. "Mr. Wallace and others" have attempted to show that this is possible by pointing to the simple and well-known fact that "no being on this earthly ball is like another all in all." They tell us that every specific character—be it of structure, colour, form, size, strength, fleetness, and so forth—is variable round the specific average or mean; so that, if it be desirable either to augment or to diminish any given specific character, there will always be one-half of the number of individuals composing the species which are varying in the required direction at the same time. Now this, of course, is sufficiently obvious; but what does it prove? It only proves what nobody, so far as I am aware, has ever dreamed of doubting—viz. that if it were to become a matter of importance in the struggle for existence that the human form, for example, should be increased as to height, short men and women would be eliminated by natural selection, while tall men and women would transmit their tallness, until in the course of many generations the race would develop into a race of giants. All this is what I would call "pure Darwinism"; and it is not apparent to me how its re-statement by "Mr. Wallace and others" has in any way "broadened" our "ideas" upon the subject. But what I have called "neo-Darwinism" is the Darwinism which fails to distinguish between such a case of the *transmutation* of a specific type in

© 1891 Nature Publishing Group